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reason why the artist should not experiment with observational moments as well as with angles of regard; it is a mistake to suppose that we are going to stay by the mask of La Mettrie after we have discovered in it the gaping fool, and it may be that we will have caught and passed it in just that instant in which it is triumphantly the laughing philosopher; every one knows that pictures have their moments of glamor, and that it is just for these moments that we love them. Why may not the artist legitimately work for these rapt eternities rather than for the tedious interregna which we fill with conscious criticism? This need not be interpreted that I pretend to understand the cubists!

III

It would offend my temperament to leave this subject without suggesting some of its implications. I have touched upon these in the field of esthetics. For psychology they are quite as interesting. Instead of dealing with perception of time and perception of space as if they were as independent as ever Kant thought them, while perception of motion follows as a sort of evolution from their fusion, ought we not to start with perception of motion (or back of that with change) and proceed thence to the explanation of time and space? It is all very well, mechanically, to treat time and space as constants of thought; but mechanics is artifice, and psychically experience seems to follow an inverse mode. The geometers, who develop the idea of space from the idea of motion, seem to have the empirical right of way.

It would be a dereliction to fail to mention Bergson in this context—and supererogation to point the application. But may it not be true that *la durée réelle* esthetizes itself in Lessingian moments? Or again may it indeed be that infinite space and all the extended splendors of the universe are but the contemplated other of the timelessly egoical absolute? For metaphysics, too, our observations seem pertinent.

HARTLEY B. ALEXANDER.

UNIVERSITY OF NEBRASKA.

SOCIETIES

NEW YORK BRANCH OF THE AMERICAN PSYCHOLOGICAL ASSOCIATION

THE New York Branch of the American Psychological Association met in conjunction with the Section of Anthropology and Psychology of the New York Academy of Sciences, on November 24 and on February 23. The November meeting was held at the psychological laboratories of Columbia University and the February meeting at the psychological laboratory of Princeton University. The following papers were presented:

Professor Thorndike's Attack on the Ideo-motor Theory: PROFESSOR MONTAGUE. (To appear in full in a forthcoming number of this JOURNAL.)

The Color Vision of Animals: MRS. CHRISTINE LADD-FRANKLIN. (No abstract offered.)

The Character of Ideas: JOHN PICKETT TURNER.

Recent psychological literature shows a wide divergence of opinion as to what the proper task of psychology should be. On the one hand psychology is defined as the science of behavior. It is claimed that the student in his investigations can totally disregard what was once thought to be the true field of psychology, states of consciousness, ideas, etc. Such is the position of Watson. On the other hand, it is said to be the task of psychology "to study the structure of the mind as if there were no such thing as body," working in the main "as though there were nothing in the world except psychic facts." Both of these positions show the unconscious influence of the Cartesian tradition,—a disposition to set ideas apart in a class by themselves. There seems to be a general disposition among psychologists to take ideas out of nature. This is unfortunate; for ideas so regarded have lost their true character. Ideas are in and of nature. They are events in nature just as much as bodies, motions, and qualities are events in nature. Events in nature come into various relations. There are qualities known as color; so there are qualities known as meaning. Certain events imply certain other events. The psychologist should study ideas as events in nature. To do this he must rid himself of the unhappy habit of associating ideas exclusively with men's bodies. The important thing is that certain events are effectively related to certain other events, cortical or others. The one thing to remember is that ideas can be successfully studied only in the concrete. Such a position calls for the reinstatement of introspection in its former place of honor.

Measurements of Judgments of Certainty: RICHARD H. PAYNTER.

The purpose of this article is to state briefly the results of a series of experiments in which measurements of judgments of certainty or accuracy of recognitions were obtained. There have been many investigations concerning judgments of various psychological processes, but judgments of certainty have received much less attention. In the present paper different degrees of certainty are used, and the validity of judgments of certainty is measured in terms of the actual facts. By this method it has been found possible to say just how much value to assign to the judgment of one's own absolute certainty or confidence. In order to compare the judgments of certainty of two or more individuals they must all have an equal number of recognitions. No attempt, however, is made to do this here. By asking the subjects to sort their recognitions of quarter-page advertisements just seen in piles of 100 per cent. certainty, 75 per cent. certainty, and 25 per cent. certainty, it is possible to measure not only the accuracy of recognitions for each of the piles, but also the judgments of the three degrees of certainty. This is done in the following manner. It is first necessary to get the per cent. of accuracy of recognitions of each pile. This is obtained by the use of Strong's formula

$$\frac{\text{correct recognitions} - \text{incorrect recognitions}}{\text{correct recognitions} + \text{incorrect recognitions}} \times 100.$$

The judgments for each of the three degrees of certainty are then obtained by calculating the per cent. the accuracy of recognitions of each pile is of the certainty or accuracy required by the pile. It was found that there are individual differences in judgments of certainty and they are largest in the lower degrees of certainty; that different degrees of certainty under the same conditions are not judged equally well; that the more valid judgments are found in the pile of absolute certainty; that the same degree of certainty is judged differently under different conditions; that the judgments are less valid in the more difficult situation; and that there is a general tendency to underestimate the high degrees of certainty and the underestimation increases with the difficulty of the task. Judgments of certainty in experiments on perception or recall memory may be measured in the same way as those in recognition memory. Finally it is of practical importance that no two statements of absolute certainty or any other degree of certainty be considered of equal value unless actually found so on measurement.

Transfer and Interference in the Substitution Test: HENRY A. RUGER.

The purpose of the study was to determine whether a well-formed rival habit or a poorly-formed one had the greater influence on the formation of a given habit. The plan of the experiment included an initial and final test series with a given key and a practise series with keys formed by varying the arrangement of the test key. For the practise series the group representing the well-formed habit practised on a single rival key; the group representing the poorly formed habit either constantly changed to a new key or practised fewer times on the same rival key. In addition to these two main groups there were three control groups and one group which practised on the test key. One of the control groups read newspapers during the practise period; another did addition, and the third worked on a different type of substitution. All the groups took the initial and final tests with the test key. All the groups did better in the final than in the initial test. However, the rival-habit groups showed much less improvement than the control groups. Consequently there was a dominant interference effect. This interference effect was greater in the group that formed the one strong rival habit than in the one that formed one or many weak rival habits. The control groups were so planned as to have different degrees of relatedness in their practise series to the test keys. The newspaper group simply read what interested them—spontaneous attention; the addition group worked with voluntary attention and at top speed. The substitution-control group worked on material similar to the test series, but not conflicting with it. The three groups followed this, the above, order in the extent of the improvement of the final over the initial test. Since the difference, however, is less than the probable error, the control groups may be considered as equivalent in this particular case. The group which practised on the test keys showed two and a half times the improvement of the control groups, while the control groups showed twice the improvement of the poorly formed rival habit group and three times the improvement of the well-formed rival habit group. Improvement was measured in terms of substitutions per second.

Three hundred and fifty subjects took part in the experiment. Woodworth's and Well's color-naming and geometrical substitution tests were employed. The symbols forming the keys were five different letters or figures.

Some Tests for Efficiency in Telephone Operators: H. C. McComas.

Two methods may be followed in testing telephone operators; one, by analyzing the activities at the switchboard and examining each, the other by testing these activities as a whole. The latter was followed in the work at the Princeton Laboratory. The apparatus duplicated an actual switchboard, on a small scale. The operator made connections at the board and these were timed by a kymograph in an adjoining room. The kymograph records showed the time which elapsed between the appearance of a light over a call connection and the moment an operator "plugged in"; also, between the moment a number was called and its appropriate connection made. Fifty records in succession were taken for each subject. The operators were ranked according to the quickness of their reactions. This ranking was compared with the composite ranking made by two telephone supervisors independently. The test easily detected the two best, and two out of three of the poorest, of the nine operators supplied by the Princeton exchange.

This rather difficult test was supplemented by one which called for very much simpler apparatus; practically a test in motor coordination. The operator sat before a table supporting an upright board upon which was fixed a sheet of paper containing ten crosses, arranged in three irregular rows. With a pencil she sought to touch the intersections of the crossed lines in quick succession. After each thrust at a cross the pencil point was brought down upon a blotter on the table. This gave a movement similar to that of the switchboard. Each subject was instructed to make the movements as quickly as possible, but not to sacrifice accuracy for time. Tests were made for each hand and with the sheets in various positions. The records in time were taken with a stop-watch; those for accuracy, by measuring the distances of the pencil marks from the intersections of the lines. The rankings thus obtained agreed remarkably well with the estimates of the supervisors, showing a correlation of .6250, with a probable error of .14 (by Spearman's Footrule). We have, then, in this form of the motor-coordination test a valuable means of detecting the quickness and accuracy of telephone operators,—two of the most important traits which make for success at the switchboard.

An Experimental Critique of the Binet-Simon Scale: CARL C. BRIGHAM.

The Binet-Simon scale was applied to 294 children from 6 to 16 years of age, the majority of cases (226) being under 12 years. Experimental conditions were adhered to as strictly as possible. The three investigators were always in ignorance of the physical age of the child being examined.

A normal distribution of cases about the "at age" position was found, 83 per cent. of the cases under 12 testing "at age," 3 per cent. "above age," and 14 per cent. "below age."

The scale was not uniform for all ages, as shown by the average age difference of each physical age group, given in the following array:

Physical age	7	8	9	10	11	12
Average difference	0	0	0	— 0.5	— 0.7	— 1.4

The lack of tests above twelve years, and the difficulty of the "twelve year" tests cause the deviations from the norm at 10, 11, and 12 years.

The teachers and the principal graded the children into five groups according to mental capacity. The average age difference of the five groups correlated with the teachers' judgments were as follows: "Very bright" + 0.9, "Bright" 0, "Average" — 0.5, "Dull" — 0.9, "Very dull" — 1.8. In 4 per cent. of the cases there was a disagreement between the judgments of the school authorities and the results of the tests.

From the results of the investigation, it was found possible to conclude that the scale, as now standardized, measured the development of intelligence of the children examined with at least 96 per cent. efficiency, and served as an adequate measure of comparatively slight individual differences in groups of the same physical age. The "twelve year" tests were found to be unsatisfactory. Sex differences were slight, girls possibly tending to vary more than boys. The influence of the personal equation of the experimenters upon the results of the tests was found to be negligible.

Recall in Relation to Retention:[1] GARRY C. MYERS.

Ten words were pronounced with regular tempo to 300 boys and girls of normal school, academy, seventh, and eighth grades. The subjects were made to believe it was a regular spelling test. At various intervals the several groups of each grade were surprised by the request to recall as many of the words as they could remember. All groups compared gave a final recall after the same interval (one hour, one-half hour, or three weeks). One group had two intervening recalls, one had one, and one had no intervening recalls.

The results for final recall are best with two intervening recalls, and for one intervening recall much better than for none. The gain by the five minute over the immediate recall is noticeably greater in its effect on the final recall, than the gain of immediate recall over no intervening recalls. The total percentages for the respective groups of girls are 89, 71, 58; for the boys, 73, 61, 52 (final recall after 30 minutes). The total percentages show a strong gain in efficiency in the final recall after one hour, as a result of immediate recall—girls, 76, 43; boys, 61, 40

On the whole the girls are noticeably superior to the boys and their mode is one degree higher for each period of time. For immediate recall and recall after one hour the mode for the boys is at 5, for the girls, at 6. After three weeks it is at 4 and 6, respectively. The average deviation from the mode is consistently greater for the girls than for the boys.

The pedagogical significance of these findings, especially in relation to drill and frequent reviews, is obvious.

A Comparison of Stylus and Key in the Tapping Test: H. L. HOLLINGWORTH.

During a prolonged series of tests both stylus and telegraph key were used in the tapping test by the same persons. The paper presented some comparison of the results secured by the two methods. Data secured by the two methods can not be treated as even qualitatively comparable,—the two methods not only do not yield the same results, but they do not seem even to test the same function. The key is much slower than the stylus, the difference increasing with practise.

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[1] This paper is published in full in the *Journal of Educational Psychology,* March, 1914.

The best individual by one method is not the best by the other. There is 20 per cent. gain as the result of practise, when using the stylus, but no gain at all in the use of the key. The variability of the records is greater with the key than with the stylus. With respect to amount of improvement through practise, individuals stand in the same relative order by the two methods, but the individual variabilities are quite different in the two cases.

The Work Curve for Brief Period of Intense Application: R. S. WOODWORTH.

Though the question of mental fatigue has been most examined in prolonged work, it is possible that a characteristic work curve should be obtained from short periods. In collaboration with Drs. Wells and Pedrick, the author has studied periods of 5–40 seconds in controlled association tests (logical relations, color naming, simple directions), series of 10 or 20 stimuli being visually presented all at once, and the subject being required to react to the stimuli one after another without intermission. The time of each single reaction was recorded in order to see whether the speed of reaction changed in the course of the series. The work curve so obtained varies from trial to trial, but on the average, runs a definite course. The initial reaction is the slowest, the next few the quickest of all, then comes a gradual decline of speed till the last reaction, which is quicker than those just before it. In the traditional language of the work curve, we find here a rapid warming-up, followed by progressive fatigue and an end-spurt. These conceptions are, however, of questionable value when applied to so brief a period of work, and a truer interpretation may be had from the notions of overlap and interference. The "fatigue effect" is here, probably, an index of the steady accumulation of interferences, while the warming-up and end-spurt effects can be connected with the overlapping of the reactions to successive stimuli. Overlap acts to the advantage of the performance as a whole, in spite of the division of attention involved; but in the case of the first reaction, the division of attention is present without any chance of gain from the overlap, while in the final reaction the division of attention lapses and the advantage of overlap remains. When the same test material is used with an interval of a few seconds between the presentation of successive stimuli, both overlap and interference would be expected to drop out; and, in fact, the work curve under these conditions reduces practically to a dead level.

A Comparison of the Influence of Strychnine and Caffeine on Mental and Motor Efficiency: DR. A. T. POFFENBERGER, JR.

The paper is based on a comparison of the results of two recent studies, namely, "The Influence of Caffeine on Mental and Motor Efficiency," by H. L. Hollingworth,[2] and "The Effects of Strychnine on Mental and Motor Efficiency," by A. T. Poffenberger, Jr.[3] Striking differences appear in the action of the two drugs upon certain mental and motor processes. The two tests were conducted on the same general plan, and comparison of the two is both permissible and easy. The tests were those well known in every psychological laboratory. Motor ability was tested by the tapping test, coordination test, and the steadiness test, while the mental ability was tested by the color naming test, opposites test, cancellation test, and calculation tests.

Caffeine caused an increased efficiency in most of the tests, the amount of increase varying with the size of the dose. Exceptions to this statement were few, the principal one being the decrease in steadiness with the increase in the size of the dose of caffeine. No after effects were noted during the course of the test which extended over a period of about forty days.

The strychnine test, covering about the same period of time, showed none of these effects, except in the case of the steadiness test where there was a suggestion of decreased steadiness after a dose. There was neither an increase in efficiency nor a retardation measurable during the period of the test.

The explanation of the difference is to be looked for in the seat of the action of the two drugs in the nervous system, the latter acting primarily on the cord and medulla and the former affecting the higher centers of the cerebrum.

H. L. Hollingworth,
Secretary.

Columbia University.

IDEALIST TO REALIST, ONCE MORE: A REPLY

In a recent number of this Journal[1] Mr. J. E. Turner makes a justifiable criticism not, I think, upon my argument against "neo-realism," but upon a questionable expression in my statement of the argument. He objects to my attributing to the realist the "certainty that he is . . . having a complex experience described by the terms yellowness, coolness, etc."[2] As Mr. Turner truly says the realist would hold that he is describing "the object, not his experience as yellow." Mr. Turner's criticism is simply met, and

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[2] "Archives of Psychology, No. 22, 1912.

[3] *Am. Jour. Psychol.*, 25, 1914, 82 ff.

[1] "Miss Calkins on Idealism and Realism," this Journal, Vol. XI., pages 46 ff.

[2] This Journal, Vol. VIII., page 453, quoted, Vol. IX., page 603. The sentence is not quoted entire by Mr. Turner.